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ATMOSPHERIC RADIATION: 1975-1983

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Rarely, in the press of day-to-day scientific work, is there a quiet moment to stand back and survey our part of the geophysical landscape. These quadrennial reviews, it seems to me, are the ideal place in which to do so. But too often we have had a deadening recitation of details rather than a lively forum. I could not write that kind of review. Atmospheric radiation is not just a collection of facts. It is a human enterprise, brimfull of the usual conflicts, triumphs, and tragedies found in any enterprise. And it operates in a larger context. I feel that it is essential to address these aspects, as well as the technical ones. What I have attempted therefore, is a *scientific essay*, expressing my own reasoned opinions and making judgements of value. Delicate issues are not bypassed, but I have tried to look at them fairly and objectively. To those who feel their work is not sufficiently showcased, I offer my apologies; but I feel we have to attend to certain large issues of great import first.

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Trace Gas Greenhouses

Trace gas greenhouse effects are one of the two theoretical problems (the other being cloud radiation) that have catapulted radiation scientists onto the center stage of atmospheric science. It began with the modern renaissance (it has had several incarnations) of the CO_2 problem in the early 1970's. That problem has mushroomed to international proportions. DOE, with shrinking budgets in most areas, has a growing budget for CO_2 studies (Oak Ridge has even established a "Carbon Dioxide Information Center"). Further impetus came from the CIAP ozone-reduction scare and then from Ramanathan's (1975) discovery of the significant greenhouse effect of Freons.

The net is now being cast more widely, as it is recognized that other trace gases -- N_2O, CH_4, volatile organics, and so on -- all have perceptible greenhouse effects (e.g., Ramanathan, 1980; Lacis et. al., 1981). Furthermore, many of these gases are increasing right along with CO_2; it is estimated that, until the year 2000, their combined greenhouse effects will pace that of CO_2 (WMO, 1982). This is because, being optically thin, their radiative effect increases linearly in their concentration, while that of CO_2, being optically thick, increases only logarithmically. And they often have much larger cross-sections per molecule than CO_2 (especially the Freons).

The trace gas problem has helped us to forge many interdisciplinary connections, wherein, I believe, lies the future of our field. The big GCM's are turning more and more to examinations of the effect of CO_2 radiative forcing on dynamics, energy transfer, precipitation, and even ocean circulation (Manabe/Stouffer, 1980; Hansen et. al., 1981). Atmospheric chemistry and stratospheric dynamics have become heavily involved, and now radiative-dynamical-photochemical models of the stratosphere are being built (e.g., Fels et. al., 1980; Callis et. al., 1983). The CO_2 problem has become such a central focus of atmospheric science that many formerly independent sheikdoms like air-sea exchange and polar science are being drawn into its orbit. This, I think, is a salutary development. It is giving us a unified view of atmospheric science, the way satellites gave us a unified view of the Earth.

Radiative-convective models (reviewed by Ramanathan/Coakley, 1978) have played a decisive role in greenhouse (and many other) studies. These models are truly one of our field's greatest success stories. They have moved out of the hands of the original experts (Ramanathan, 1976) and are now used widely (e.g. Hummel, 1982, Chylek/Kiehl, 1981). They tell us that the *major* trace gases -- H_2O, CO_2, and O_3 in order of importance -- contribute about 30 K to the global average surface temperature. Minor trace gases contribute another 2 K or so. The longwave and shortwave effects of stratospheric O_3 change on surface temperature roughly cancel, but the two effects work in the same direction for tropospheric O_3, which, because of pressure-broadening, is as important as stratospheric O_3. (Ramanathan and Dickinson, 1979, give a particularly thorough treatment of the O_3 question and introduce the intriguing Fixed Dynamical Heating concept). Stratospheric water vapor, which exerts a dramatic effect on surface temperature, may not be as rock-steady as we have imagined; in fact, it may slowly rise if, as many believe, it is controlled by the temperature of the tropical tropopause.

The importance of looking at greenhouse perturbations at the tropopause, rather than at the surface, is still not fully grasped by everyone. Newell and Dopplick (1979) created a flap by backsliding to a pure surface radiation point of view, which has been refuted at some length in a very interesting paper by Ramanathan (1981). However, an important caveat is that, on a regional basis, tropopause radiative forcing may not be felt at the surface if (a) direct radiative connection is blocked (for instance by clouds or the H_2O continuum), and (b) transport is vigorous enough to move the heat out before it can be convectively communicated to the surface. This is exactly what happens in the tropics, according to the new TEC model of climate (Hoffert et. al., 1983).

The cause of the H_2O continuum remains controversial. At a conference of experts on water vapor (Deepak et. al. 1979), many rejected the dimer explanation on the grounds that concentrations were too small. They preferred the wings-of-strong-lines explanation, and in fact LOWTRAN and FASCODE (discussed below) contain a theoretically-derived continuum from 0 to 20000 inverse cm based entirely on line wings. Carlon (1981) has kept the dimer explanation alive, however, arguing that the low concentrations deduced for dimers do not account for the match-making role of ions in the atmosphere.

Kaplan emphasized at a meeting in 1980 that the H_2O continuum in the 15-micron band of CO_2 mutes the CO_2 greenhouse effect. In 1981, not knowing of Kaplan's work, I discovered a factor of 10 difference between my own and Ramanathan et. al.'s (1979) calculations of tropical surface radiation sensitivity to CO_2 doubling. It turned out that I automatically had the continuum in the 15-micron region, by virtue of using LOWTRAN, whereas Ramanathan did not. Subsequent in-depth re-examination of the problem by Kiehl and Ramanathan (1982) revealed a much smaller (25%) effect at the tropopause than at the surface. This is an example of the surprises that may still await us in seemingly well-tilled ground like clear-sky IR models.

A more intense trace-gas greenhouse on early Earth also seems the best resolution of the 'faint early Sun paradox' (e.g. Kuhn/Atreya, 1979; Owen et. al., 1979), although it seems rather miraculous that these gases would go away at the right rate to keep the Earth from overheating and destroying all life. It is also hard to imagine any other than a greenhouse explanation for the presence of liquid water on early Mars (Cess et. al., 1980; Hoffert et. al, 1981).

The activity in the area of spectroscopy and remote sensing of trace gases is intense (e.g. Goldman et. al., 1983; Menzies et al., 1981). It now seems possible to monitor these species with high accuracy, even remotely. Experimentally, we have an embarrassment of riches.

The significant remaining problems in the trace gas area seem less purely radiative -- although for many of the more exotic minor trace gases, line parameters and often even band strengths remain unknown -- than interdisciplinary, involving chemistry and dynamics. Many non-greenhousing trace gases can chemically alter concentrations of absorbing species -- e.g., CO can oxidize (in the presence of NO) to form tropospheric O_3. And trace gases can feed back on each other through dynamics -- e.g., when CO_2 warming of the tropical tropopause increases concentrations of stratospheric H_2O.

For review articles, WMO (1982) gives an excellent bird's-eye view; Logan et. al. (1981) looks at chemistry; and Bach (1976) relates pollution to climatic change.

The "McClatchey Tape" and LOWTRAN

One of the most important chapters in atmospheric radiation history was written during the last decade by McClatchey and his colleagues at AFGL. They entered a fragmented field -- molecular spectroscopy -- and left it in such order that we now take for granted getting the latest line parameter tape or the latest version of LOWTRAN or FASCODE from the National Climatic Center.

Things were not always so easy. During the 1950's, Kaplan and Plass did not even include water vapor absorption in their studies of the IR effect of CO_2 changes. Much of the data was suspect, there were great spectral gaps with no data whatsoever, and one had to laboriously assemble what data there was from diverse sources. McClatchey and his group resolved to improve the situation. They marshalled all the existing absorption line data, from the microwave to the visible, and targeted key spectral regions that needed work. Then they either did that work themselves, or let contracts to do it. And with amazing speed they put together the first uniform compilation of the important parameters -- wavenumber at line center, intensity, half-width, ground-state energy, quantum numbers -- for every line that was relevant to the Earth's atmosphere (including those for isotopes). That was 1972. By now (Rothman, 1981), they are up to some 159,000 lines. As previous line parameters are proven inaccurate by new measurements, the tape is corrected; and, over the years, many new lines have been added as well.

This means that IR modelers can now generate their own band models, with any spectral resolution they choose. But if they can live with either 5 or 20 inverse cm spectral intervals, there are two more AFGL products of great value -- LOWTRAN5 (Kneizys et. al., 1980) and its 5 inverse cm cousin (Robertson et. al., 1981). These give transmission and radiance for arbitrary slant paths in the atmosphere using a band model of King fitted from the line parameter data. Refractive geometry, aerosol extinction (Shettle and Fenn, 1979), and the famous "McClatchey atmospheres" are available as options.

I was one of the earliest users of LOWTRAN (LOWTRAN2, then), and it enabled me to make a unified treatment of shortwave and longwave radiation, which until then had been regarded as distinct subjects. Thus I view LOWTRAN not merely as a great convenience, but as an important unifying influence in our field.

For line-by-line calculations of transmission and radiation, AFGL has developed FASCODE (Smith et. al., 1978; Clough/Kneizys, 1979; Clough et. al., 1981). FASCODE, as its name implies, is designed for speed. By decomposing the line profile into four basis functions, it neatly sidesteps an old problem -- that narrow lines at high altitudes require a ridiculously dense spectral mesh at all altitudes. FASCODE convolves any sort of slit function with the data, among other features. It is the Cadillac of line-by-line models, except for its radiance calculation, which uses a very crude integration, like that of LOWTRAN. Scott and Chedin (1981) have developed a competing model which may be faster, if not as versatile, for some applications.

The original line parameter tape included 7 absorbing species. Partially in response to the trace-gas greenhouse problem, this has been supplemented by a new trace gas tape (Rothman et. al., 1981) with line parameters for 13 more species -- NO_x, SO_2, ClO, OCS, and other gases suspected of affecting the ozone layer. The new tape is much less complete than its Big Brother, but data for it is being generated at a furious pace, as a scan of the "trace gas" section of the Bibliography will indicate.

Another great benefit of the AFGL work is the provision of a standard against which everyone can measure their work. In the past, one could only compare against measurements which, for all one knew, were as erroneous as one's own. Now there is a well-policed, scrupulously maintained, and constantly updated data set -- a worthy opponent against which to pit one's own measurements (Ben-Shalom et. al., 1980, 1981; Flaud, et. al., 1980; Skinner/Nordstrom, 1976). By this process, the AFGL products are continually improved, to the benefit of us all.

Cloud Radiation

Cloud radiation leapt to prominence in the early 1970's when GARP planning documents identified it as one of a handful of critical issues. This stimulated a small flood of theoretical studies, as well as some new measurements. Theoretical work divided into the following general areas:

- microphysical -- the direct impact of radiation on droplet growth
- macrophysical -- calculate optical properties (albedo, etc.) given the microphysics
- plane-parallel and finite cloud models
- enhancement of cloud absorptivity
- spectrally-detailed models with realistic atmospheres
- parameterizations based on liquid water content and sun angle
- cirrus clouds
- cloud-climate feedback
- radiation-dynamics interactions in stratiform cloud.

Cloud physicists remain skeptical of Barkstrom's (1978) idea that cloud droplets grow much faster near the cloud top as they are exposed to 'the cold of space' in the 8-12 micron window. But cloud physicists, like dynamicists, have a history of dismissing radiation. That is probably why they have difficulty explaining aspects of cirrus and other extended cloud forms. I suspect we will see much more on radiation-microphysics in the future.

Most cloud-radiation models are plane-parallel. This is the natural milieu in which to test many hypotheses about cloud radiation. However, there has been an explosion of papers in 3-D (finite) clouds, mostly cubical in shape (McKee/Cox, 1976; Aida, 1977; Wendling, 1977; Davies, 1978; Liou/Ou, 1979; Harshvardhan et. al., 1981; Welch/Zdunkowski, 1981; Ellingson, 1982; Bradley, 1982). Monte Carlo and 3-D Eddington methods are the primary radiative transfer tools used. The earlier papers proclaimed loudly that finite clouds had lower albedos than plane-parallel clouds, because of leakage out of the cloud sides. This was no great surprise. More recent contributions (e.g. Davis et. al., 1979) have tried to find how to normalize plane-parallel results to agree with the finite cloud results, which seems more productive. Lately, the emphasis has been on studying cloud-cloud interaction and the approach to the plane-parallel limit as intercloud gaps narrow.

The actual or implied denunciation of plane-parallel cloud modeling in some finite cloud papers requires comment. First, measurements are the acid test of any model; it is not enough that a model simply 'looks' better. Perhaps a plane-parallel model taking proper account of vertical inhomogeneity will agree better with measurements than typical cubic cloud models with their spatially-invariant liquid water and drop distributions. Perhaps weighting plane-parallel albedos by the proper measure of cloudiness fraction will correctly predict the albedo of patchy cloud fields. But more importantly, our job is not to make our models as complicated as nature herself; it is to simplify and idealize, in order to gain understanding. Plane-parallel cloud modeling is an entirely acceptable way to do this. And, on a practical level, (a) we will never know, or want to know, the shape and size of every single cloud on Earth, and (b) plane-parallel clouds can be modeled with a level of spectral and angular detail unreachable in finite cloud models. Our job is to learn how to make simple adjustments to plane-parallel predictions to mimic patchiness, not to reject this very valuable modeling approach out of hand.

Observations of broad-band cloud albedo and absorption are not very incisive in testing theoretical models. Therefore, special emphasis has been laid on predicting cloud absorption; and here we have found significant disagreements. Theory (Twomey, 1976; Liou, 1976) finds cloud absorptions no higher than 20%, as Fritz found in the 1950's. Yet some observations have been as high as 40% (Reynolds et. al., 1975). Three explanations have been offered: leakage out cloud sides, absorbing aerosols, and very large drops. Leakage undoubtedly explains the largest disagreements with theory (Ackerman/Cox, 1981). But Twomey's (1977) conclusion that aerosols are unimportant may have to be revised in light of recent findings of worldwide soot pollution, in combination with the Chylek/Srivastava (1973) mixing rule predicting a possible dramatic enhancement in the absorption of soot-water mixtures. The Welch/Cox (1980) conclusion that very large drops could increase absorptivity to over 30% has been muted upon using a more realistic drop distribution (Wiscombe et. al., 1983) so there probably isn't much mileage in this explanation.

Spectrally-detailed models were developed first by myself in the early 1970's (Wiscombe, 1975) and later by Dave/Braslau (1975), Twomey (1976), Liou (1976), and Stephens (1978a). Gaseous absorption within and above the cloud was included, although with varying degrees of sophistication, ranging from a grey-gas assumption to exponential fits. These models are essential to a proper understanding of cloud radiation, since the solar spectrum, Rayleigh scattering, ozone absorption, and water liquid and vapor absorption all vary dramatically with wavelength, making the cavalier spectral averaging characterizing earlier work untrustworthy.

Cirrus clouds were identified as sensitive regulators of surface temperature in the famous radiative-convective paper of Manabe and Wetherald. Special attention has therefore been paid to them, although it remains difficult to conduct field programs at such high altitudes, as has been done by Griffith et. al.. (1980) and Paltridge/Platt (1981). Platt (1978, 1979, 1981, etc.) has done a number of lidar studies as well as making theoretical calculations. Liou and his students (Roewe/Liou, 1978; Freeman/Liou, 1979) continue to make theoretical calculations of cirrus radiation in both the shortwave and longwave regions, as has Stephens (1980a,b). The main impression is of the great variability in cirrus optical properties: emissivity can range from near zero in sub-visible cirrus to near unity in tropical cirrus, and albedo from near zero to as high as 50% or so. Clearly, in view of the recent recognition of the ubiquitousness of cirrus (which was formerly missed by, or invisible to, ground observers), this problem is worthy of continued effort.

The debate continues over whether or not clouds have a radiative feedback effect on global climate. Cess (1976, 1982), using broad-band satellite data, found the albedo cooling and greenhouse warming cancelling each other when cloudiness increased. Wetherald/Manabe (1980) found essentially the same result in a GCM with interactively-predicted clouds. On the other hand, Hartmann/Short (1980) and Ohring/Clapp (1980) found the albedo effect winning by at least 2 to 1, although they used narrow-band satellite data to make broad-band inferences, for which Cess has criticized them. Charlock (1981) and Stephens/Webster (1981) incorporated interactive clouds into radiative-convective models, finding in the first case a negative feedback (compared to prescribed clouds) and in the second case such a bewildering variety of behaviors that no definitive conclusion about the sign of the feedback could be made.

Partly in order to pin down cloud-climate feedbacks, the International Satellite Cloud Climatology Project has been launched. Under its aegis, a new surface cloud climatology is being assembled as well, by London and Warren. The goal is to obtain cloud fraction and cloud height, and to this end a competition between a large number of cloud retrieval schemes has been held, with a final scheme consisting of the best parts of the best methods having recently been settled upon.

In the late 1960's, Lilly was forced to put cloud-top radiative cooling into his boundary-layer stratus model in order to keep the stratus from dissipating. This gave instant legitimacy to an idea that had been advanced by radiation scientists like Möller as early as 1951. There is now a lively debate among PBL theorists, for example as to whether a vertically distributed radiative cooling rather than Lilly's delta-function is necessary (Deardorff/Bussinger, 1980; Lilly/Schubert, 1980; Randall, 1980). Meanwhile, Herman/Goody (1976) have suggested a shortwave greenhouse as the cause of observed layering in Arctic stratus; and Fravalo et. al. (1981) have observationally and theoretically demonstrated the importance of both shortwave heating and longwave cooling in controlling cloud-top entrainment. Webster/Stephens (1980) and Griffith et. al (1980) discuss vast areas of long-lived mid- and upper-level cloudiness in the tropics, whose longevity could only be due to radiative forcing.

There is now a much more receptive environment for radiative-dynamical studies in connection with extended cloudiness (although cloud physicists continue to ignore this type of cloudiness). It may well turn out that neither the formation, nor the persistence, nor the dissipation of extended clouds can be explained without radiation.

Experiments on cloud radiation have been less frequent than model studies. Fortunately, Cox (1976 and many others), Herman (1977, 1980), Derr, Ellingson, Stephens et. al.(1978) and others have not just taken new measurements of cloud radiation, but have moved us closer and closer to the Complete Radiation Experiment, in which cloud microphysics, temperature, humidity, and even aerosol content are measured simultaneously. They have assembled an impressive collection of data which modelers really should have a look at.

It would be impossible to discuss all the new data. Let me merely cite subject areas where significant new observations have been obtained:

- tropical cloud systems -- in GATE, MONEX, and EPOCS (Derr/Gunter, 1982)
- cirrus optical properties (discussed above)
- Arctic stratus (Herman, 1977, 1980; Wendler et. al., 1981; Tsay/Jayaweera, 1983)
- South Polar clouds (Smiley et. al., 1980)
- cloud particle sizes and shapes from Knollenberg probes
- re-evaluation of ice refractive index (Warren, 1983)
- complete re-measurement of water refractive index (e.g. Downing/Williams, 1975)

In connection with the last two items, those who are still using the old Irvine-Pollack compilation, thinking it makes no difference, would be well-advised to reconsider their position.

The hassles involved in such experiments are hard for theoreticians to imagine. In GATE and MONEX, for example, radiation missions were often assigned low priority, or scrubbed altogether. Observational programs require immense dedication and are not nearly as productive of publications as yet-another-multiple-scattering-model. Yet they remain the life blood of our field.

Earth Radiation Budget

Earth radiation budget, and in particular the three-satellite ERBE program scheduled to fly next year (Woerner, 1979; Barkstrom/Hall, 1981, 1982; Hall 1982), is the third great problem which has brought atmospheric radiation to prominence. Dave Atlas has called ERBE "the most visible climate-related project in the world." If one can find the net radiation at the top of an atmospheric grid box from a combination of spacecraft, one immediately has the sum of the heat storage in, and the heat transport into and out of, that grid box. Since the present surface and upper-air network, already costing about $1 billion, is not likely to grow much, especially in the Southern Hemisphere, and since that network only gives reliable estimates of heat transport in part of the Northern Hemisphere mid-latitudes, the incentive for a successful ERBE experiment is obvious.

Therefore every effort is being made to make ERBE a first-class experiment. Many areas which got short shrift in the past, due to budgetary and other constraints, were singled out for special attention, including:

- optimal orbit configurations
- detailed instrument modeling and error analysis
- calibration against international standards, and in flight as well
- time- and space-averaging procedures (with special emphasis on the diurnal cycle)
- modeling of the angular variation of the outgoing intensities, both empirically and theoretically
- inversion of measurements at satellite altitude to 'top-of-atmosphere'
- correlative measurements (from the ground, from aircraft, and from other spacecraft)
- exceptionally thorough scrutiny of the data for errors (Hall, 1983)
- data archiving procedures (with special emphasis on making the data easily available and useable by outsiders).

The small bibliography does not begin to reflect the intense activity in some of these areas; it is simply that much of the work has not yet found its way into the journals.

A particularly important aspect of ERBE is its concern for the post-experimental use of the data. In the past, it had been assumed that researchers outside the tightly-knit satellite community -- for example, experts in weather prediction, climate, and radiation -- would automatically pick up Earth radiation budget data and use it. Things didn't work out that way. A few climate theorists used the zonally-, annually-averaged data, and still fewer used the zonally-, seasonally-averaged data, but this did not amount to widespread usage. So ERBE, by assembling an international 'Science Team' from universities and non-satellite government laboratories, is trying to assure that the data gets used to, among other things: improve and/or validate radiation models and simple radiation parameterizations in climate models (e.g. Slingo, 1982); correlate radiation budget with observed meteorological variables -- in both directions; study diurnal cycles; and better understand the role of clouds. This is as important as the taking of the data itself.

In some ways, the ERBE Science Team is a stalking horse for the entire atmospheric science community. That community has not made much quantitative use of satellite data. By inducting the team members into a satellite program from its very inception, and letting them have a say in how it is run (through Working Groups to which each member is appointed), it is hoped that the barriers which have existed may be broken down.

Meanwhile, the Earth radiation budget data from Nimbus 6 (Smith et. al., 1977; Jacobowitz et.al., 1979; Campbell/Vonder Haar, 1980; Bess et. al., 1981) and Nimbus 7 (Hickey et. al., 1980) has been and continues to be analyzed. The CSU school (Oort/Vonder Haar, 1976; Ellis et. al., 1978; Campbell/Vonder Haar 1980) continues to add to its laurels, culminating most recently in the fine summation of Stephens et. al. (1981).

Simple narrow-to-broad-band conversion algorithms (Gruber, 1978; recently improved for the IR by Abel/Gruber, 1979) continue to be applied by NOAA-NESS to the operational polar orbiter data to generate radiation budgets (Winston/Gruber, 1979). This continues to irritate some climate theorists who feel that this type of conversion is unwarranted. Nevertheless, after some study of the problem, I am convinced that it is warranted; it just needs to be done better, and with more channels. The Europeans are already doing it (Gube, 1982). Suomi's VAS instrument on GOES, with some 18 narrow channels, for example, would probably give more than enough information to characterize the entire spectrum. And there are many other narrow channels on other satellite systems -- Tiros, Landsat, DMSP -- which could be used as well.

On the subject of narrow bands, Ramanathan (1979) raised a lonely voice calling for some *restricted* channels (e.g. the CO_2 15-micron band) as well as broad-band channels on future Earth radiation budget experiments. I would like to add my voice to his. His argument is simply this: the climate may change -- for example due to CO_2 -- without giving any signal in the total longwave flux to space. But there will be a signal in restricted bands. This is most definitely worth looking for, especially since, in the Workshop on First Detection of CO_2 Effects held last year, every surface-based measurement of climate change had charges of ambiguity laid against it.

There are some very positive developments taking place in the satellite area, signalled not only by ERBE, but by the appearance of more sophisticated theoretical and experimental analyses of problems like angular modeling (King/Curran, 1980; Davis/Cox, 1981; Smith/Green, 1981). Equally important, perhaps, is the appearance of this work in the journals -- a welcome break from satellite tradition. After a decade of heavy reliance on empiricism, the new emphasis on advanced modeling and mathematical techniques is sorely needed.

Aerosol Radiation

To many outside our field, there seems an overemphasis on aerosol radiation (the Bibliography contains some 250 entires in this area!). In truth, many within the field have voiced the same opinion. The small (< 1%) changes in planetary albedo wrought by stratospheric aerosols hardly seemed worth the flood of papers on this subject (see Cadle/Grams, 1975; Coakley, 1981). Normal tropospheric aerosols have larger optical depths; but with a washout/rainout time of 1-2 weeks, Bryson's Human Volcano seems unlikely to materialize. And one would have to assume fixed cloudiness to assign aerosols an unambiguous role in climatic change, except in cloudless regions like the Rajasthan Desert in India, studied by Bryson.

Nevertheless, there are important reasons for pursuing aerosol radiation studies. The first is that we have had four minutely-examined volcanic explosions -- Agung, Fuego, St. Helens, and now El Chichon -- providing unparalleled opportunities for observing short-time-scale climatic change caused by reduced insolation to the troposphere (e.g. Pollack, 1976; DeLuisi/Herman, 1977; Russell/Hake, 1977; Cess et. al., 1981; Ogren et. al., 1981; Howard, 1981; Mitchell, 1982). These are the only global-scale climate-change experiments available to us (Hansen et. al., 1978), except for the few-tenths-of-a-percent flickering of the solar 'constant' and the much longer-term CO_2 effect. Such events warrant intense concentrations of effort.

The second reason is that mankind may be adding a very insidious and powerful absorber to the natural tropospheric aerosol -- soot. (Gray (1976) has proposed that we do this on purpose, for weather modification.) This idea is not new -- Weinman looked at it in the late 1960's -- but it was only recently that the incredible ubiquitousness of soot became known. Very few laboratories had been able to analyze aerosol samples for soot -- mass spectrometers miss it. Those few that did -- especially Rosen et. al. (1978) and Charlson -- began finding soot everywhere but Antarctica. There is now a program to study its effect in the Arctic (Shaw/Stamnes, 1980; Rosen et. al., 1981; Porch/MacCracken, 1982; Cess, 1983) where it may reach optical depths of 0.2. And if it is scavenged by falling snow, it can reduce snow albedo by 10-20% or more (Warren/Wiscombe, 1980). An entire conference (Novakov, 1979) has been held on "Carbonaceous Particles in the Atmosphere."

The third reason is that global climate models have lacked any aerosol influence in their radiation parameterizations, much less any aerosol feedback, as for example in altering cloud microphysics. Yet even background aerosol effects are larger, in flux units, than the perturbations like $2xCO_2$ that have been examined. Reck (1976) and Charlock/Sellers (1981) have looked at aerosols in the context of radiative-convective models, which is the normal first step before trying to put them in more complex models.

The fourth reason is that aerosol pollution of all sorts substantially alters the urban boundary layer through radiative-dynamic interactions (e.g. Welch/Zdunkowski, 1976; Venkatram/Viskanta, 1977).

The Workshop on Light Absorption by Aerosols (Hindman/Gerber, 1981) brought experimenters with a wide variety of instrumental techniques together to measure imaginary refractive index of several precisely-monitored aerosol types. Errors of 1/2 an order of magnitude and more were common, and it was not clear who was right. This sort of activity is of much greater value, at this point, than further aerosol-radiation modeling studies.

Characterization of the size distribution has advanced beyond the "Junge Era" (cloud radiation is still stuck in the "Deirmendjian C.1 Era"). It is recognized that the highly absorbing particles (soot, hematite) are concentrated in a sub-micron mode, while the more transparent particles (silicates and sulfates) are concentrated in a second, large-size mode (Lindberg/Gillespie, 1977). Optical properties of many common aerosol materials have been measured as a function of wavelength (see especially the series of papers by Patterson), although not with the spectral thoroughness of water and ice,which are easier to study because perfect plane surfaces can be obtained. Growth of hygroscopic aerosols with relative humidity has been modeled and measured (Zdunkowski/Liou, 1976). All in all, knowledge of aerosols advanced so rapidly that already in 1976 it was possible to parameterize their radiative effects (Toon/Pollack), although this has been largely superseded by the remarkably comprehensive effort of Shettle/Fenn (1979).

Pressing problems include mixing rules for the average radiative effects of soot/non-soot mixtures (the conventional ones don't predict as much absorption as is observed -- see Ackerman/Toon, 1981); scattering and absorption by non-spherical particles (discussed below); and gas-to-particle conversion processes, such as apparently generate the Antarctic and stratospheric aerosols. Another interesting area is the coupling between radiation and dynamics in dusty situations; this has been conclusively demonstrated during Martian dust storms (Zurek, 1978), and on Earth there have been many arguments that dust stabilizes the lapse rate.

Single Scattering

Mie theory remains the backbone of our treatment of single scattering. New algorithms for exact Mie scattering (Wiscombe, 1980) are enjoying wide popularity. Some people are even doing single scattering calculations on microcomputers (Barber, 1981)! Even in cases where we know the particles to be nonspherical, Mie theory is still often the best approximation. Unless the particles have one specific shape, or are preferentially oriented by aerodynamic forces, like ice needles or plates, it is very difficult to take any credible account of non-sphericity which improves on Mie theory. Pollack and Cuzzi (1980) have proposed a simple scheme to adjust the Mie single-scattering albedo and asymmetry factor, but the calculations of Mugnai and myself show these adjustments to fail in general. Other schemes for modifying Mie theory have met with still less success.

Nevertheless, non-spherical particle scattering has become a significant focus of activity. (Liou introduced the subject into atmospheric science in the early 1970's, by approximating cirrus particles as infinite cylinders.) In 1979, Schuerman (1980) hosted a meeting on the subject which brought together atmospheric scientists, electrical engineers, and experimentalists for the first time. Electrical engineers had been solving scattering problems for a variety of odd shapes since the early 1960's (e.g. Uslenghi, 1978; Morgan/Mei, 1979). We have adopted the best of their techniques, for example, the EBCM method of Waterman as extended by Barber/Yeh (1975).

A singular triumph was the exact solution of the spheroidal scattering problem (Asano/Yamamoto, 1975; Asano/Sato, 1980). The formulas are fiendishly involved, and apparently run into numerical difficulties for size parameters greater than 30. But then, so did Mie calculations in the early days. I have no doubt these numerical problems will be solved, although, what with the immense computational expense of averaging over orientation, spheroidal calculations will never become as widespread as Mie calculations. Probably the first reasonable non-spherical adjustments to Mie theory will come from a careful study of spheroidal scattering.

In one way, spheroids are special, however: they are convex. This is what led Mugnai and I to study wavy-surface particles, with mild concavities. We found these concavities to have a striking effect. Since many natural aerosol particles have rough surfaces (to say nothing of those which are honeycombed with voids), it is likely that the qualitative effects of non-sphericity are not going to be entirely revealed by studying the spheroidal case alone. In particular, if cavities and voids trap radiation, like a black-body cavity in the laboratory, particle absorptivity may be enhanced.

The other shape for which a new solution has been derived is the hexagonal column (Wendling et. al., 1979; Coleman/Liou, 1981; Cai/Liou, 1982). Within this solution must lie many of the most interesting phenomena of meteorological optics, which are usually analyzed with simple ray-tracing (Tape, 1980). However, after looking at the many naturally-occurring shapes of ice crystals in the atmosphere, I would say that we still have a long way to go in characterizing ice-particle scattering. Weil/Chu (1980) have given approximate solutions for ice crystal plates which are thin relative to the wavelength. Measurements of ice crystal scattering are much rarer, because the crystals fall so fast, but Sassen has studied the problem in a series of papers, and Winchester and Jayaweera have made measurements which are unpublished at the time of this writing.

Chylek, in a series of papers, has looked for simple ways to approximate various features of non-spherical scattering without solving Maxwell's equations exactly. Whitney (1979) has proceeded in somewhat the same vein, invoking an entropy-like principle, but I confess to not understanding her paper in a quick reading.

Two techniques have been developed for studying single-particle scattering. One is to build microwave-sized analogues, which was invented in the U.S. but languished here while the Europeans (Zerull, 1976) took the lead. Schuerman et.al (1981) revived the U.S. effort, but this has been cut short by Schuerman's untimely death.

The other technique is 'optical levitation' (Ashkin/Dziedzic, 1980, 1981; Grehan/Gouesbet, 1981). Here, the particle is suspended by radiation pressure in a vertically-pointing laser beam -- no strings, wires, or spider webs! Incredibly fine details of the scattering process can be monitored in this technique.

Mie theory itself continues to be a subject of study. 'Complex angular momentum theory' (Nussenzveig, 1979) has finally cracked a problem that eluded even Van de Hulst (1981) -- namely, the correct large-radius asymptotic formulas, including the surface-wave terms. The new formulas are excellent approximations and are free of the annoying 'ripple' that plagues exact Mie calculations. By including just 4 or 5 terms in the CAM expansion, accurate results can be obtained down to size parameters of 15-20 (Nussenzveig/Wiscombe, 1980).

Standard 'mixing rules' for calculating the mean refractive index of a heterogeneous particle have recently been challenged by Chylek/Srivastava (1983). (Niklasson (1981) and Bohren/Battan (1980, 1982) have also re-examined mixing rules.) If this work is correct, mixtures of soot with non-absorbing material can be much more absorbing that previously thought.

In 1979, an entire issue of J. Opt. Soc. Amer. was devoted to a conference on meteorological optics. This is a rather curious 'field', populated mostly by hobbyists who love it but earn their keep doing other things. I couldn't help wondering how some of these optical phenomena might be used for remote sensing, but perhaps that would require a degree of pattern recognition and color discrimination found only in the human eye. Considering the expense of mounting field experiments to look at ice crystals and water drops in situ, however, perhaps it would be worth a try.

Multiple Scattering

I must admit that my own early experiences in radiative transfer have left me with a somewhat jaundiced view of what does or does not constitute an original contribution in this field. In my first job, in 1969, I was immediately thrown into a problem of time-dependent spherical radiative transfer. A problem in time-dependent 2-D cylindrical radiative transfer quickly followed. In all this, I was tutored by old hands like Burt Freeman, veterans of Los Alamos and Livermore who had been grappling with very nasty radiative transfer problems for years. Thus, it has been hard for me to watch some of my colleagues in atmospheric radiation reinventing the wheel.

Nevertheless, there are unique aspects of atmospheric radiation which these people never dealt with. First, they used scattering iteration methods (like Herman et. al., 1980), which restricted them to optical depths less than about 10. They had nothing like adding-doubling. Second, their absorption coefficients varied smoothly with wavelength; they did not have to deal with line structure. Thirdly, they dealt mostly with isotropic and linearly anisotropic phase functions, not our sharply forward-peaked ones. (I directed much of my own research into those three areas.)

The key discoveries of the 1960's were: doubling, by Van de Hulst; adding-doubling, by Grant/Hunt; and Fourier expansion in azimuth, by Dave. By 1971, both the adding-doubling and spherical harmonics (Dave, 1975) methods were fairly mature. But spherical harmonics exhibited both spurious absorption and wild oscillations in the intensity as a function of angle. These problems have only been remedied recently (Karp et. al., 1981). Therefore adding-doubling became the method of choice, not only because of its stable numerical properties, but because its variables -- for example, reflection matrices -- had simple, direct physical interpretations. Liou revived Chandrasekhar's discrete ordinates method in 1973, but because it also experienced numerical ill-conditioning (in computing eigenvalues), and because the physical interpretation was more obscure, it was not widely adopted. Its defects, also, have only been remedied recently (Stamnes/Swanson, 1981; Stamnes/Dale, 1982).

In order to obtain intensities which vary smoothly in angle, Davies (1980), Karp (1981), and Stamnes (1982) all recommend the old trick of iterating the source function. Davies gets the source function just from the delta-Eddington approximation, and, considering that delta-Eddington was only designed to give accurate fluxes, obtains remarkable accuracy in computed intensity (better than 10%).

Various techniques were put forward (Wiscombe, 1976a; Twomey, 1979; Cogley/Bergstrom, 1979) for including thermal emission in scattering calculations. More important than the techniques themselves, was what they signalled: that we were no longer going to let the IR be the province of pure absorption. We wanted to know how scattering media like aerosols and clouds behaved in the IR.

When I first went to NCAR, I met a solar physicist who crowed about how advanced the astrophysical radiative transfer techniques were, compared to my own humble fumblings. Nevertheless, as they stood, his techniques were ill-suited to atmospheric problems. Barkstrom (1976) has taken the best of the astrophysical ideas and developed a technique which is suited to the atmosphere. It is numerically stable and particularly useful for strong spatial inhomogeneity.

Lenoble (1977) performed an exceptional service by undertaking an intercomparison of many exact and approximate methods for 5 standard problems. I and many others submitted results to this 'competition', and for the first time we could all see that we were calculating fluxes accurately to about 3 significant digits, and intensities to 2 (except for the Monte Carlo methods). This seems sufficient for atmospheric work.

We have seen really important breakthroughs in simple approximations for highly anisotropic phase functions. It began with the extended two-stream (Coakley/Chylek, 1975) and delta-Eddington (Joseph et. al., 1976) methods and more or less culminated in the unifying analyses of Meador/Weaver (1980) and Zdunkowski et. al. (1980). Two important purposes were served: first, simplification, which often leads to better understanding; and second, providing a useful tool to climate modelers and other non-experts, who had great need of it. And, on the subject of approximation, Ronnholm et. al. (1980) have reminded us that when the properties of the medium are only known to within a certain error, it makes no sense to hone our radiative transfer methods to a fine edge.

The time has come, it seems to me, to turn our attention to other areas than monochromatic, plane-parallel multiple scattering, lest we be accused of beating that problem to death. One such area is the incorporation of a rough surface like the ocean (Plass et. al., 1975, 1976; Fung/Eom, 1981). Another is spherical geometry, for cases where the Sun is near the horizon, as in the polar regions (Leung, 1976; Simmoneau, 1980). Yet another is 2-D (Crosbie/Dougherty, 1981) or 3-D (Crosbie/Schrenker, 1982; Kimes/Kirchner, 1982; Ou/Liou, 1982) radiative transfer, for dealing with horizontal inhomogeneity. And I would emphasize that I am not just thinking of finite clouds; other quantities, like aerosol, surface albedo, even temperature, sometimes have sharp horizontal gradients as well.

Another area needing further elaboration is spectral integration across a band of absorbing lines. The two seemingly distinct methods for doing this -- exponential fitting (Wiscombe/Evans, 1977; Evans et. al., 1980; Chou/Arking, 1981) and photon path distribution -- were shown to be essentially the same by Bakan (1978). Mostly, the pressure scaling approximation is used for dealing with inhomogeneous paths, and Chou/Arking have shown how to make the very best possible scaling approximation, rather than just using fixed powers of pressure and temperature the way LOWTRAN does. Still, there is much about these methods we do not understand: their error compared to line-by-line calculations, their suitability for very broad spectral intervals, the limits beyond which they cannot be pushed.

In closing, I am happy to announce that multiple scattering theory has been experimentally confirmed (Graber/Cohen, 1975)!

Measurement

Until recently, radiation measurements could only be relied on to a few percent. Even international standards disagreed until the late 1960's. That is why I am particularly impressed by the cavity radiometers of Hickey (1980, 1982) and Willson (1981) on Nimbus 7 and the Solar Maximum Mission respectively. These measured the solar 'constant' variation to 0.1% or so, and incidentally found the sun to be flickering at the 0.3% level over periods as short as 30 days! This is an impressive achievement, especially in the harsh environment of space, where instruments notoriously degrade. Their work has allowed climate modelers to get on with more pressing problems without worrying, but not knowing, how the Sun was changing. (Hoyt (1979) has also reviewed Abbott's old work on solar flickering.)

The 1970's can truly be said to be the decade when atmospheric scientists, like astronomers, began using almost the whole electromagnetic spectrum. The Backscattered UltraViolet (BUV) Experiment probed the stratosphere from space. Visible imagery made great strides; Landsat enables us to examine individual puffy Cu with 80-m resolution, and it is said that DoD satellites can see a tank on the ground

Near-IR channels were added on NOAA and DMSP satellites for snow-cloud discrimination. Clusters of 4- and 15-micron channels were used to retrieve temperatures and humidities. METEOSAT brought us our first moving pictures of the upper atmosphere at 6 microns. Microwaves, originally used just for sensing sea ice, now bring us liquid water, precipitation, soil moisture and snow measurements. And SEASAT carried side-looking radar to profile the sea surface (Lipes, 1982). These are but a sample of the achievements.

Many of these same wavelengths are also being used for surface- and aircraft-based measurements of: temperature (Murray, 1980), clouds, humidity (Buck, 1976), trace-gas concentrations, sensible and latent heat flux, rain (Wang, 1980), wind (Eberhard/Schotland, 1980), divergence, turbulence, presure (Gardner, 1979), aerosols, and PBL height. And they are often giving values averaged over space, helping to eliminate meteorology's age-old problem of trying to depict spatially ragged fields with point measurements. Atlas and Korb (1981) predict the ultimate replacement of the $1 billion surface network with "a composite of passive and active sensors in the visible, IR, and microwave."

The number of clever ways in which lasers are being used is awe-inspiring (Grams, 1978; Russell et. al., 1982). From the original few fixed wavelengths, a wide spectrum (literally) of possibilities has evolved. Some lasers are even tuneable, within limits! Besides the original 'monostatic' configuration, where laser and receiver coincide, new bistatic and even tristatic arrangements are being pioneered (Abreu, 1981). Dual-wavelength (DIAL) setups have been developed near the oxygen band at 0.76 microns and elsewhere (Browell, 1979), which offer particular promise for space-based remote sensing. And Raman scattering, at a wavelength slightly displaced from that of the laser itself, while too weak for distant detection, is an ideal way of fingerprinting many trace gases (Petri, 1982). (Applied Optics has become the focal point for most of this literature.)

Doppler radar has apparently been a quantum leap forward in the detailed mapping of storms. Knollenberg optical probes are working a similar revolution in cloud physics, allowing vast amounts of droplet and ice particle size and shape information to be collected automatically, rather than by the painstaking manual methods of the past.

I was particularly taken with the 'bugeye' instrument (Davis/Cox, 1981, 1982) for snapshotting the intensity field in 12 solid angles simultaneously. The bugeye was used extensively in MONEX to develop typical models of cloud and surface angular scattering. It avoids cosine reponse problems and, because of the simultaneity, is ideally suited to aircraft and satellite platforms.

Many of the original satellite instruments were merely imagers. It was almost impossible to calibrate their shades-of-grey into radiometric units. That is changing now. For one thing, there are more shades available, as 6-bit data is replaced by 8- and even 10-bit words. And several groups have calibrated GOES, Landsat, and METEOSAT against aircraft, surface, and other satellite measurements (Kriebel, 1981; Duggin, 1981; Koepke, 1982; Beriot, 1982). The trend away from mere pictures and toward quantitative radiation measurements is a healthy one.

High on the list of remaining problems is to take lots more spectrally detailed measurements, from all platforms. The ones from Nimbus 3 and 4 were tremendously exciting, and were crucial in validating the Ellingson/Gille (1978) longwave model. Similarly, spectrally-detailed measurements of snow albedo (O'Brien/Munis, 1975) were vital in the inference of soot in snow (Warren/Wiscombe, 1980). There is much, much more to be learned in the spectral detail.

Improved measurement accuracy is going to be vital in the future. The present 1-3% is not going to be sufficient for examining subtler radiative effects in the atmosphere. Already, for cloud absorption, some deduced values come out negative due to cancellation of all significant digits in differencing the measurements. Perhaps some entirely new technology is needed; perhaps the old type of radiometer is inherently imperfectible. Those radiometers convert radiation into temperature changes. What we should be looking for is other, more accurately measurable material properties into which the radiation can be converted.

Surface Reflectivity

There have been several new compilations of surface albedo for the entire Earth (Robock, 1980; Kukla/Robinson, 1979; Hummel/Reck, 1979). This is much to be applauded, since for years almost everyone used the same one or two data sets, and there was a false sense of security that we really understood surface albedo. The three new data sets exhibit significant differences, which more accurately reflects our uncertainty.

We have learned that desert albedos can be as high as 40-50%, and that they reflect more in the near-IR than in the visible (Rockwood/Cox, 1978; E. Smith, 1981). This causes 'heat lows' over most deserts; they reflect away so much solar, and emit so much longwave, that they actually suffer a radiation deficit. High desert albedos were adduced as a causative factor in the Sahelian drought (Berkovsky, 1976; Charney et. al., 1977; Norton, 1979). Cess (1978) also examined surface albedo as a climatic feedback mechanism, but in connection with the biota, and on Ice-Age time scales.

Models of some types of surface albedo have continued to improve. They all treat the surface as an absorbing-scattering medium. Application was made to dusty surfaces (Egan/Hilgeman, 1978) and to pure snow (Wiscombe/Warren, 1980), among others.

Much more needs to be learned about the spectral and angular variation of natural reflectivities. Models indicate that the usual deviations from isotropic reflection can be important (Fitch, 1981). The effect of surface roughness needs to be better known, from the scale of capillary waves on the ocean (Sidran, 1981), to forest canopies (where the leaves are scattering 'particles', see Cooper, 1982), to mountain ranges. Theoretical modeling of shadowing and multiple scattering among surface 'facets', to say nothing of light upwelling from below in the case of snow, sea ice, forest and ocean, is not very advanced (e.g. Choudhury 1979). Carroll (1982) has made some progress with a triangular-waveform model of snow-surface roughness. The electrical engineers have developed very sophisticated surface-roughness theories over the years (Beckmann/Spizzichino, 1963), but for perfectly-conducting surfaces; these are of some, but not much help.

Odds and Ends

"No one wants their field parameterized" -- so said an oceanographer at a recent meeting. This seemed like a reasonable sentiment at the time; oversimplification can offend one's sense of the elegance and subtlety of one's field. Yet radiation scientists have developed many parameterizations (Lacis/Hansen, 1974; Stephens, 1978b; Chylek, 1978; Liou/Wittman, 1979; Leighton, 1980; Twomey/Bohren, 1980; Thompson/Warren, 1980), mostly for the IR and for clouds. Why are we so willing to parameterize?

Upon reflection, it became clear that the reason is: we recognize the importance of interfacing with other fields. In order to do so, we must simplify our subject to the bare bones, so that it may run fast on a computer, or be no more complex than other pieces in a large model, or be useful for back-of-the-envelope estimates in a classroom or a field experiment. And we prefer to do this simplification ourselves, as the ones best qualified. This signifies a certain Whole-Earth view, a sense of connection with other fields, which is still lacking in oceanography.

An important advance in our field has been the achievement of a unified treatment of shortwave and longwave problems. Even through the 1960's, shortwave and longwave experts kept to their own turf. Their methods were entirely distinct. But when I began building the ATRAD model in 1970 (Wiscombe, 1975), I was unaware of these distinctions. Using exponential fits and LOWTRAN, I developed a methodology which was the same in all spectral regions -- even the microwave. This kind of unification has continued to the point where most of the younger radiation scientists are able to work in any region of the electromagnetic spectrum.

This is absolutely necessary nowadays. Climate problems involving cloud or stratospheric ozone changes lead to compensatory changes in shortwave and longwave fluxes; you cannot just look at one or the other. Carlson/Benjamin (1980) found that, as the amount of Sahara dust over the GATE area increased, there were big changes in both the shortwave and longwave fluxes to space -- which cancelled almost perfectly! And most remote sensing will be done, in the future, with multi-spectral strategies.

It struck me forcefully, in assembling the Bibliography, that, compared to European scientists, we in the U.S. are much more enamoured of models. As a result, I see rather an unhealthy imbalance between theory and experiment. It is true that lidar is a beehive of activity, but not everything can be measured with lidar. Many experimentalists have expressed discouragement that, while theoreticians grind out papers at a furious pace, their work necessarily is much slower to appear, making them seem unproductive. If it is true that we have come to place a higher value on theoretical than on experimental work, then certainly we, as scientists, have completely lost touch with our roots.

The publication glut in our field has become absolutely unbearable. A few of us, in satellite radiation, do not publish enough, at least in the archival literature. But for too many others, gone are the days when a scientist waited to publish until he had something important to say. Now, many articles and reports are little more than our version of time cards.

Too many of us are shotgunning the literature, perhaps in search of lax editorial standards. But getting an article published in some outlying journal seems a Pyrrhic victory, at best. We should rather concentrate our papers in the main outlets for atmospheric radiation research -- JAS, JAM, Applied Optics, JQSRT, and QJRMS (everyone wants to publish at least once in QJRMS; such is its cachet). Only by doing so do we have *any chance at all* of keeping abreast of one another's work.

It is always a pleasure to welcome new books into our field. Among many, I might mention Liou (1980), our first advanced textbook on atmospheric radiation; Paltridge/Platt (1976), with more emphasis on IR radiation, dynamics, and climate than Liou; Twomey (1977), on the mathematics of remote sensing; Slater (1980), on remote sensing instrumentation; and McCartney (1975), suitable for an introductory course, with more material on cloud and aerosol microphysics than the others.

Strangers in a Strange Land

Many radiation scientists feel, upon picking up atmospheric science literature, like scientists in general feel when they first pick up Science magazine. The title does not prepare them for the narrow focus, which in atmospheric science is dynamics (and in Science is biology). Shibboleths like 'baroclinic instability', 'potential vorticity', and 'primitive equations' seem to fill the literature, and often there is nary a photon to be found anywhere. Hence the title of this section.

Until about 1968, when the climate revolution struck, there were really very few atmospheric radiation scientists. They could all have met in a small room. But already by 1972, when Tom Vonder Haar inaugurated the U.S. national radiation meeting, a very large room was needed. For the fifth such meeting, next fall in Baltimore, we have received over 150 abstracts. Thus we are now a substantial component within atmospheric science, although our growth has pretty much leveled out of late.

But our prestige has not grown with our numbers. There are prevailing attitudes that only dynamics problems are (a) really difficult, or (b) worthy of an atmospheric scientist's attention. The first seems especially strange to many of us, who migrated into atmospheric science from physics, mathematics, and engineering, where standards of difficulty are highly developed. Dynamics problems do not seem any more or less difficult than radiation problems to us, their vector analytic formulation notwithstanding; they are merely different. I, for one, have my Ph.D. in fluid dynamics, but I have always found radiation problems more challenging and more interesting.

And as to what is worthy of an atmospheric scientist's attention, that depends on your perspective. Agreed, if you want to forecast mid-latitude cyclones, you had better pay close attention to your dynamics. Radiation, slow albeit inexorable in its effects, can safely be neglected. Indeed, as the April 1, 1983 issue of Science announced, the NMC forecast model "cranks out weather in perpetual darkness."

But the matter is entirely otherwise if your focus is climate; or polar meteorology; or tropical meteorology; or the stratosphere; or long-lived stratiform clouds; or anywhere where fast-breaking baroclinic waves do not grab all the headlines. In these areas, radiation is either important, or very important. Many of us work in these areas; and we think they are just as deserving of respect as short-term mid-latitude dynamics.

Most of the rest of us work in remote sensing. Just as the 1970's was the "Decade of Climate," so, I believe, the 1980's and 1990's will be the "Decades of Remote Sensing." It is the only way out of the overwhelming data-dearth dilemma. But for years remote sensing researchers have complained that they are treated like servants in the house of atmospheric science -- like mere purveyors of an engineering product. They would like it known, as would I, that remote sensing is a first class theoretical and experimental problem,every bit as deserving of respect as baroclinic wave studies.

It would seem only prudent for atmospheric science as a whole to open its doors and grant full citizenship to those of us in radiation, climate, and remote sensing. We are, after all, carrying some of the torches which will light the way into her future. And we have no desire whatsoever to remain 'Strangers in a strange land.'

Bibliography

The bibliography herewith consists of some 1300 items, arranged in groups more or less corresponding to the section headings in the review. Restriction even to this large size was only possible by insisting that: (a) a paper had to have radiation as its main focus (although I have seeded some papers about using radiation to measure other atmospheric variables); (b) it had to be in JAS, JAM, MWR, Bull. of the AMS, QJRMS, Appl. Opt., JOSA, JQSRT, or Tellus. Fields like lidar, radar, inversion techniques, etc. are merely skimmed. Each now warrants its own large bibliography. Including them would make the reviewer's task not merely daunting, but impossible (unless, of course, AGU wants to create full-time reviewing positions). I direct your attention particularly to the 'Miscellaneous' category. Therein may lie the spores of some of the future 'hot' problems we will be dealing with.

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THE DYNAMICS OF LARGE SCALE ATMOSPHERIC MOTIONS

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Introduction

Dynamic meteorology is the study of atmospheric motions associated with weather and climate. Traditionally dynamical meteorologists have emphasized motions of synoptic and planetary scale. During the past four years there have been a number of exciting developments in the dynamics of synoptic and plantary scale motions. Many of these are related to various aspects of short term atmospheric variability both internally and externally generated. A number of studies have gone beyond the traditional perturbation approach in which disturbances are imposed on a zonally symmetric basic state. Significant progress has been made in understanding the dynamics of the quasistationary zonally asymmetric flow and its control of the transient circulations.

In this review we focus on the progress made in the understanding of synoptic and planetary scale motions in the troposphere. Although many important advances have been made in stratospheric dynamics, that area is covered in another review (Hartmann, 1983) thus, stratospheric problems will be considered here only in terms of the links between the stratosphere and the troposphere.

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Linear Instability Studies

The study of baroclinically unstable flows remains a central theme of theoretical dynamics. In recent years the stability of realistic mean zonal flow profiles with both latitudinal and vertical mean wind variations has been studied using both initial value and eigenvalue techniques. An important conclusion of these studies is that planetary scale (wavenumber 1-3) disturbances can be baroclinically unstable in the presence of realistic wind profiles on the sphere, and that such modes can have very large vertical scale and, hence, may account for the observed transient eastward propagating long waves of the Southern Hemisphere winter stratosphere. (Hartmann, 1979; Straus, 1981). It has been shown that the discrepency between these results and earlier beta-plane idealizations is a consequence of the specification of fixed meridional scales in the beta-plane models (Hoskins and Revell, 1981).

At the other end of the spectrum of baroclinic instability, progress has been made in